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the industry and ingenuity with which the process of matching has been carried on." Prof. George S. Huntington also recognizes the difficulty in his admirable paper on certain muscular variations in the Transactions of the New York Academy of Sciences. "I believe that we are right," he says, "in referring such variations * * * to the development of an inherent constructive type, abnormal for the species in question, but revealing its morphological significance and value by appearing as the normal condition of other vertebrates." But if so are we justified in calling them 'reversions?' Dr. Huntington's views do not seem to differ widely from those that I expressed in a paper on this subject in the *Naturalist*, of February, 1895. "Those very irregularities, which we call abnormal, point to a law in accordance with which very diverse animals have a tendency to develop according to a common plan." I do not need to be told that even to establish a law (and I have only hinted at one) is not in the least to show how it acts. All that I claim is that some other principle than atavism must be invoked. The pitiable abuse of it is shown in a book that I met the other day on the vermiform appendix. After stating that this is to be considered as the end of the cæcum, the author went on to remark that the rare cases of a double appendix, which are said to have occurred, are presumably to be explained by the double cæca found in many birds. Dr. Frank Baker, in the April number of the *Anthropologist*, severely criticises similar abuses.

The question is associated with another of very general importance, namely, whether similarity of structure is necessarily evidence of descent or even of relationship. One would think from certain writings that it is conclusive; but, of course, every anatomist knows that it is not. It seems that similar special organs, or arrangements of structures, occur in widely different orders in species of similar habits or surroundings. Mr. Dobson* instances a South American rodent with the habits of moles in which the arrangement of the muscles of the leg is the same as that of the true moles. This clearly points to a law which, it seems to me, the occurrence of anomalies tends to confirm. It is

* Jour. Anat. and Phys., Vol. XIX.

in the hope of having this discussed that I lay it before the readers of SCIENCE.

THOMAS DWIGHT.

'PROGRESS IN AMERICAN ORNITHOLOGY. 1886-95.'

IN the *American Naturalist* for May (Vol. XXX., pp. 357-372) Dr. R. W. Shufeldt gives, under the above title, a statistical summary of the new American Ornithologists' Union 'Check-List of North American Birds,' with criticisms *passim* on various points, followed by an arraignment of the Committee which prepared it for ignoring all recent work on the classification of birds, there being no change in this respect from the 1886 edition. He proceeds to enumerate, for the benefit of this Committee and others, the various 'elaborate classifications of birds' and the various authors who have written on the taxonomy of birds, not omitting to mention, of course, those of Dr. Shufeldt. No doubt great advances have been made in the last ten years in the knowledge of the structure and relationships of various groups of birds; and while many moot questions remain, and authorities still differ respecting the propriety of many of the recently proposed changes, a few points may be considered as having been practically settled. While it might have been well enough for the Committee to have expressed its opinion on some of the questions thus raised, such a procedure, in view of the still very unsettled state of the subject, seemed not particularly called for; especially as there were practical difficulties in the way of introducing any change in the order or succession of the higher groups.

Dr. Shufeldt strangely overlooks the main purpose of the new Check List, which was not, as he seems to think, the incorporation of the various species and subspecies added during the last ten years, and the changes of nomenclature introduced during the same period, scattered through half a dozen supplements to the original list; while this was important, its main purpose was the revision of the matter relating to the geographical distribution of the species and subspecies, which the interval of ten years had rendered, in many instances, not merely imperfect, but absolutely erroneous and archaic. Yet this feature of the new edition seems to

have escaped Dr. Shufeldt's notice, so greatly is he shocked by the lack of taxonomic revision.

In all Check Lists of North American Birds, from Baird's, published in 1858, down to Ridgway's and Coues' lists of 1880 and 1882, the species are numbered in an orderly sequence; and the numbers serve an important function, they being often used in the place of the names, not only in labeling specimens, particularly eggs, but extensively in correspondence between collectors, the number serving as a convenient symbol for the name. Hence it is important that they be given the greatest possible permanency. The A. O. U. Committee recognized this fact in preparing the Check List, and devised a scheme whereby any number of interpolations could be made without disturbing the notation of species already in the list. Of course, a transposition of groups would necessitate a new notation and create endless confusion and inconvenience, for which the Committee would receive condemnation compared with which Dr. Shufeldt's strictures can be easily borne, particularly since his views on several points are not extensively shared by other equally competent taxonomers.

The greater part of Dr. Shufeldt's paper consists of a detailed comparison of the two editions of the check list, with an analysis, taking the birds by ordinal or family groups, of the changes introduced in the 1895 edition. This is a useful statistical résumé for those interested in the subject.

It is, however, not free from typographical errors, nor from others that by no stretch of courtesy can be placed in that category. For example, *Megascops flammeola idahoensis* is recorded (p. 361) as *M. a[sio]. idahoensis*; the subgenus *Burrica* is mentioned (p. 365) as *Barrica*; it is said (p. 366), 'subgenus *Parus* inserted' in the 1895 edition, whereas it is given in the 1886 edition as well; on p. 368 the statement about the Swallow-tailed Gull is the exact reverse of the truth. His method of noting changes in the status of species or subspecies tends to a wrong conception of the facts in the case. Under 'species omitted' and 'species added,' etc., he places not only species omitted or added, as the case may be, but forms

whose status has merely been changed from species to subspecies, or the reverse. Thus, as in the case of *Zonotrichia intermedia*, for example, where the change is from specific to subspecific rank, the change could have been easily and correctly indicated by a formula like the following: *Zonotrichia intermedia* (1886) = *Z. leucophrys intermedia* (1895). In place of this *Z. intermedia* is placed under 'species omitted' and *Z. leucophrys intermedia* in the list of 'subspecies added;' whereas, so far as the number of forms is concerned, there is neither omission nor addition.

In a footnote to p. 364 we find the following: "The Starling (*Sturnus vulgaris*) essentially gained a place and recognition in the A. O. U. 'List' from the fact that it has been successfully 'introduced' from abroad. If this be granted, the Committee were guilty of very unscientific practice when they omitted the English Sparrow (*Passer domesticus*) from the 'List' (also *Passer montanus*), and it can only stand as an example of how far men will allow their prejudices to carry them and blind their scientific instincts." If the critic of the A. O. U. Committee had taken the trouble to refer to the 1886 edition he would have found that the Starling was introduced in the first edition of the 'Check List' on the basis of its occurrence in Greenland, and that his presumptuous criticism and moralizing about 'prejudices' were wholly without cause. Since the publication of the first edition the species has been 'introduced, by importation in numbers from Europe, and appears to have obtained a permanent foothold here—a fact it seemed worth while to mention in the second edition of the 'Check List.' No 'introduced' species has been introduced in the Check List, which is intended to be what its name purports—a list of North American birds. Of late years many species of foreign birds have been 'turned out' in various parts of the United States and Canada, but with what results it is impossible as yet to determine. Dr. Shufeldt will find, however, in the 'Abridged Edition' of the 'Check List,' published in 1889, a list of 'Introduced Species,' ten in number, which at that time were known to breed in this country in a wild state. But this list forms no part of the Check List proper.

The above reference to the Starling in Dr. Shufeldt's paper, taken with other passages in the same article, clearly reveals the animus of his critique.

J. A. ALLEN.

'WHAT IS TRUTH?'

In all our speculations concerning nature what we have to consider is the general rule. For that is natural which holds good.

Aristotle, *Parts of Animals* III., II., 16.

Knowledge is a double of that which is.

Mr. Bacon in *Praise of Knowledge*.

Nature means neither more nor less than that which is.

Huxley, VII., p. 154.

If the author of the letter on 'The Material and the Efficient Causes of Evolution' (*SCIENCE*, p. 668), will refer to an article which the Editor asked me to give him, and printed in *SCIENCE* in February, 1895 (Vol. I., No 5, p. 125), I think he must admit that I, at least, have not committed the blunder which he lays to the charge of certain unspecified 'Neo-Darwinians' and 'Neo-Lamarekians,' and that there is no just *cause or reason* why my name should be dragged into print in this connection.

However, I heartily agree with him that rigorous exactness is necessary in the use of philosophical language; and I also agree with him that, when no qualification is used, or implied, the English word *cause* should mean 'that which produces a thing and makes it what it is;' although it is one thing to define a word and quite another thing to show the existence of any corresponding reality.

As I am advised by this writer to consider Aristotle and be wise, I refer the reader to the passage I have put at the top of this letter, for it shows that this great naturalist is in accord with Bacon and Huxley in the opinion that our business in this world is to learn all we can of the *order* of nature, leaving to more lofty minds the attempt to find out what it is that 'produces a thing and makes it what it is,' and every other 'necessary condition of truth' except evidence.

This correspondent says the word *conceive* is not used with precision in my assertion that, evidence seeming adequate, I believe things which I cannot conceive. As Huxley has never

been accused of inexactness in the use of words I call attention to the following passages which show that this cautious thinker also believed what he could not conceive.

"I cannot conceive how the phenomena of consciousness are to be brought within the bounds of physical science," IX., III., 122.

"I believe that we shall, sooner or later, arrive at a mechanical equivalent of consciousness, just as we have arrived at a mechanical equivalent of heat," I., VI., 191.

W. K. BROOKS.

MAY 4th, 1896.

THREE SUBCUTANEOUS GLANDULAR AREAS OF
BLARINA BREVICAUDA.

TO THE EDITOR OF *SCIENCE*: Though the subcutaneous glands in *Soricidæ* have received much attention, these structures are not so well known in all details that further observations on the subject can be considered superfluous.

In examining perfectly fresh individuals of the common short-tailed shrew, *Blarina brevicauda*, taken in midwinter, when glandular development or activity is presumably less evident than it becomes during the rut, I find three large glandular areas—a lateral pair and one infero-median.

On each side of the body, midway between the fore and hind limbs, may easily be recognized a glandular area, half an inch long and one-half as wide, in part overlying the posterior border of the thorax, and thence extending over the abdomen. This is observable without dissection; for, on blowing aside the long hairs which cover it, the space appears to be naked, though it is in fact clothed with short adpressed colorless pelage, like that on the dorsum of the manus. Small flakes of the inspissated secretion may be noticed; but the glandular orifices are too minute to be made out, even with a hand lens, though these may become more readily discernible at another season. Nor is any musky odor perceptible in the present specimens.

The third glandular area of this shrew is larger than the lateral ones, and this is the fact to which I may direct particular attention. This additional patch is situated on the median line of the belly, opposite the lateral tracts, and